DOCUMENT RESUME

ED 176 398 EA 012 124

AUTHOR Park, Rolla Edward; Carroll, Stephen J.
TITLE The Search for Equity in School Finance: Michigan School District Response to a Guaranteed Tax Base.
INSTITUTION Rand Corp., Santa Monica, Calif.
SPONS AGENCY National Inst. of Education (DHEW), Washington, D.C.
REPORT NO R-2393-NIE/HEW
PUB DATE Mar 79
CONTRACT 300-76-0065; 400-76-0160
NOTE 37p.; For related documents, see EA 012 122-123; Some tables may not reproduce clearly due to small print

EDRS PRICE MF01/PC02 Plus Postage.
DESCRIPTORS *Educational Finance; Elementary Secondary Education; *Equalization Aid; Expenditure Per Student; *Finance Reform; Foundation Programs; *School District Spending; State Aid
IDENTIFIERS Guaranteed Tax Base Programs; *Michigan; State Matching Grants

ABSTRACT

Part of a three-volume report on the effects of school finance reform, this volume examines the effects of reform on Michigan school districts' budgets from 1971 to 1976. Econometric models were used. Researchers found a very small "price" effect--an elasticity of -.02. The data provide no evidence that state matching grants stimulate school district expenditures enough to be a useful policy tool. It was also found that state block grants exert a very small influence on school district expenditures. Results imply, however, that expenditures are increased by 32 to 38 percent of the amount of state or federal categorical aid. Researchers found significantly higher expenditures in school districts serving populations that are wealthier, higher income, employed in higher-level positions, more minority, and more urban. Expenditures are significantly lower in school districts serving populations tending to live in owner occupied housing and tending to be in poverty. (Author/JM)

****
* Reproductions supplied by EDRS are the best that can be made *
* from the original document. *
****

ED176398

THE SEARCH FOR EQUITY IN SCHOOL FINANCE: MICHIGAN SCHOOL DISTRICT RESPONSE TO A GUARANTEED TAX BASE

PREPARED FOR THE NATIONAL INSTITUTE OF EDUCATION AND THE U.S. DEPARTMENT OF HEALTH, EDUCATION AND WELFARE

U.S. DEPARTMENT OF HEALTH, EDUCATION & WELFARE
NATIONAL INSTITUTE OF EDUCATION

THIS DOCUMENT HAS BEEN REPRODUCED EXACTLY AS RECEIVED FROM THE PERSON OR ORGANIZATION ORIGINATING IT. POINTS OF VIEW OR OPINIONS STATED DO NOT NECESSARILY REPRESENT OFFICIAL NATIONAL INSTITUTE OF EDUCATION POSITION OR POLICY

ROLLA EDWARD PARK, STEPHEN J. CARROLL

R-2393-NIE/HEW
MARCH 1979

EA 012 124

Rand

2

This work upon which this publication is based was performed pursuant to Contract No. 400-76-0160 with the National Institute of Education and Contract No. 300-76-0065 with the Office of the Assistant Secretary of Education, U.S. Department of Health, Education, and Welfare. The opinions and conclusions expressed herein are solely those of the authors, and should not be construed as representing the opinions or policy of any agency of the United States Government.

LC Card No. 79-12408
ISBN 0-8330-0120-5

Published by The Rand Corporation

PREFACE

With support from the National Institute of Education (Contract No. 400-76-0160), The Rand Corporation analyzed the effects of school finance reforms recently implemented in five states--California, Florida, Kansas, Michigan, and New Mexico. The study has two main objectives: (1) to describe the pre- and post-reform school finance outcomes in the individual states and to compare the differential outcomes of the reforms they adopted; and (2) to estimate the effects of fiscal and other factors on school districts' budgetary behavior. The overall purpose of the study was to provide information to officials in states that have yet to undertake or to complete reform and to federal officials with responsibilities in the area of school finance.

The study results are reported in three volumes. This report documents the analysis of school districts' budgetary behavior. Because the California, Florida, Kansas, and New Mexico reform plans constrain school districts' revenues and/or tax rates, the analysis focuses on Michigan districts. Econometric models are used to estimate the effects of different types of grants (federal or state, categorical, unrestricted block, or matching), the "prices" (or matching rates) of revenues per pupil to districts and to households, and the characteristics of districts' populations on expenditures per pupil. The work was partially supported by the Educational Policy Research Center for School Finance and Governance established at Rand under Contract No. 300-76-0065 with the Office of the Assistant Secretary of Education.

A companion report, *The Search for Equity in School Finance: Results from Five States*, R-2348-NIE, presents the descriptive analysis. It examines, for each state, (1) pre- and post-reform distributions of revenues, expenditures, and tax rates; (2) the degree to which reform has been accompanied by an increase in intrastate fiscal neutrality; and (3) the kinds of districts that have benefitted most (or least) from reform. It also compares these outcomes among the

states, offers some general conclusions regarding the effects of these states' reforms, and speculates on why the reforms led to the observed outcomes.

The third volume, R-2420-NIE, *The Search for Equity in School Finance: Summary and Conclusions*, summarizes the study's principal findings. It includes the major results of both the descriptive analysis and the analysis of districts' budgetary behavior.

SUMMARY

This study examines the budgetary behavior of Michigan school districts over the 1971/72 through 1975/76 school years. Michigan provided general aid to the schools through a foundation plan in the first two years and through a guaranteed tax base (GTB) plan in the last three years. We estimate school districts' responses to the fiscal incentives implicit in each plan. We pool cross-sectional and time-series data in econometric models that relate expenditures per pupil to different types of grants (federal or state, categorical, unrestricted block, or matching), to the "prices" (or matching rates) of revenues per pupil, and to characteristics of districts' populations. Our data are for 451 districts in each of the five years.

We find a very small "price" effect--an elasticity of -.02. A very large change in the matching rate--from 0 to 1--would stimulate additional expenditures of only $12 per pupil in the average district. The Michigan data provide no evidence that state matching grants stimulate school district expenditures enough to be a useful policy tool. We also find that state block grants exert a very small influence on the levels of school district expenditures; each dollar of grant money increases expenditures by only 6 cents per pupil on average. On the other hand, our results imply that expenditures are increased by 32 to 38 percent of the amount of state or federal categorical aid. We find significantly higher expenditures in school districts serving populations that are: wealthier, higher-income, employed in higher-level positions, more minority, and more urban. Expenditures are significantly lower in school districts serving populations that tend to live in owner-occupied housing and tend to be in poverty.

ACKNOWLEDGMENTS

We are indebted to Dr. James L. Phelps, Associate Superintendent for Research and Administration, Michigan State Department of Education, who provided the data used in this study. Our Rand colleagues Richard V. L. Cooper and Richard L. Fernandez offered useful comments on an earlier draft of this report.

CONTENTS

PREFACE iii

SUMMARY v

ACKNOWLEDGMENTS vii

Section
I. INTRODUCTION 1

II. THE DATA 6
PRICE to School Districts 6
Price to Representative Household 9
State and Federal Aid 10
Other Variables 12

III. ESTIMATED EXPENDITURE EQUATIONS 14
Cross-Sectional Regressions 14
Pooled Time-Series, Cross-Section Regressions 18
Structural Change 22
Discussion 24

REFERENCES 29

I. INTRODUCTION

This study examines factors that influence Michigan school districts' budgetary behavior. We estimate the effects of different types of grants (federal or state, categorical, unrestricted block, or matching), of the "prices" (or matching rates) of revenues per pupil of districts and to households, and of the characteristics of districts' populations on expenditures per pupil. Our models combine cross-sectional and time-series data for 451 school districts in each of five years: 1971/72 through 1975/76. These include all Michigan unified school districts for which complete data were available in all five years.*

Prior to 1973/74, Michigan distributed general purpose aid to school districts through a foundation plan. Each district received an unrestricted block grant equal to the difference (if positive) between a stipulated amount per pupil (the foundation) and the product of a computational tax rate and the district's tax base per pupil.** In 1973, the state introduced a guaranteed tax base (GTB) plan, under which the state guaranteed each district $38 per pupil for each mill of tax effort up to 22 mills. A district levying less than 22 mills received per-pupil matching grants equal to its tax rate times the difference (if positive) between $38,000 and its per-pupil tax base. A district levying 22 mills or more received unrestricted block grants per pupil equal to 22 times the difference (if positive) between $38,000 and its per-pupil tax base.

The GTB plan changed the form of state aid to school districts in ways that affect the districts' incentives to spend money on education.

---

*Michigan operated about 530 unified districts during this period. Districts excluded from our sample were generally small, rural districts for which Census data were not available.

**In 1972/73, for example, districts received (per pupil) the difference between $715 and the hypothetical proceeds of a 20 mill tax if their tax bases were less than $17,750 per pupil. Districts with per-pupil tax bases in excess of $17,500 received (per pupil) the difference between $644 and the hypothetical proceeds of a 16 mill tax.

By matching locally raised tax dollars with state aid, the plan effectively lowered the price of expenditures to school districts. In theory, we would expect a district faced with a lower price to "buy" more educational expenditure. The effect of any such plan depends on the magnitudes of responses by districts to these incentives.

School districts' budgetary responses to GTB-like school finance plans are a central concern in the public policy debate over the reform of state school finance systems. The 1971 *Serrano* decision focused national attention on the inequities of the school finance systems, and political and legal challenges to the existing systems were soon under way in most states. Since then, at least 22 states (as many as 30 by some counts) have substantially modified their systems. In many of the reform states, however, presures have emerged for further reform, or for a shift to a new reform approach, or for a return to the pre-reform system; and pressures for reform continue in most of the states that have yet to revise their systems. Recently, the federal government has entered the debate: The Impact Aid Program now permits states that have "adequate" school finance equalization programs to count a district's impact aid grant as state aid; the Congress has provided grants to assist states in planning for equalization; and direct federal incentives to states that reform their systems were discussed in both the executive branch and the Congress during the past year. The question of what constitutes an appropriate state school finance system is now, more than ever, an important public policy issue.

The array of finance plans variously termed guaranteed tax base, district power equalizing, or variable matching systems* play a prominent role in the school finance debate. These plans provide more equal fiscal opportunities to school districts while allowing the provision of public services to reflect local preferences. Any district

*The terms "guaranteed tax base" or "variable matching" are generally used in reference to a formula that ensures each district a minimum revenue per pupil for each unit of tax effort. "District power equalizing" (DPE) generally refers to a plan that also allows each district no more than the guaranteed amount of revenue per unit of tax effort. That is, a DPE plan is a GTB plan with full recapture of "excess" local revenues.

can obtain the same revenues per pupil as any other district* if it levies the same local property tax rate, regardless of any differences in their property tax bases or other characteristics. At the same time, local governments are able to select whatever level of service they (their constituents) desire and are willing to support.

But GTB plans are risky. Depending on school districts' responses to a plan, the state may confront an aggregate aid obligation that exceeds its ability (or willingness) to support the schools. Alternatively, it may find that school spending, in particular districts or in the aggregate, falls below what it considers to be the socially (or politically) desirable level. Distributional issues also arise. Low-income communities may not be able to afford the tax rates they would have to levy to keep pace with higher-income communities. What if disproportionately minority communities systematically levied lower tax rates than those of heavily white communities? In either case, the responses may reflect communities' local preferences and budget constraints. But the argument that low-income (or minority) communities were free to respond like their higher-income (or majority) counterparts and would have obtained the same per-pupil revenues if they had done so, may not satisfy those concerned with the distribution of public services among different populations. For that matter, an egalitarian theme runs through much of the reform debate, and many reform advocates would object to substantial interdistrict differences in school spending even if they were not systematically related to districts' characteristics. The problem of school districts' responses to the incentives inherent in a GTB plan is central to the debate over what constitutes an appropriate school finance system.

Recent studies by Feldstein (1975) and Ladd (1975) estimate school district expenditure equations using data from Massachusetts. Their results, partially reproduced in Table 1, indicate a fairly

*Fiscal opportunities are equalized up to a point if, as in Michigan, there is an upper bound on the tax rate beyond which the guarantee no longer applies. Districts whose tax bases exceed the guarantee continue to have a fiscal advantage in the absence of a "recapture" provision.

Table 1

RESULTS OF PREVIOUS STUDIES

	Source			Source	
Variable[a]	Feldstein[b]	Ladd[c]	Variable	Feldstein	Ladd
CONSTANT	N.R.[d]	-0.057	RESIDENT	-0.118	-0.309
		(0.03)		(2.74)	(2.66)
GROWTH	-0.336		Y	0.475	0.459
	(4.42)			(6.99)	(2.90)
STBLK	0.066	0.030	PRIVATE	-1.112	-0.018
	(5.08)	(1.70)		(2.84)	(0.78)
CAT	0.136	N.R.	POVERTY		0.078
	(5.04)				(1.65)
PRICE	-1.000	-0.485	PROF		0.102
	(5.41)	(1.63)			(1.52)
AV	0.283	0.239	R-Squared	0.643	0.65
	(7.45)	(2.53)			
			SEE	N.R.	0.017
PUP/POP	0.208	-0.027			
	(1.49)	(0.36)	N	105	78

NOTES: Dependent variable is E, operating expenditures per pupil. All variables are in natural logs. T-statistics are given in parentheses.

[a]Variables are as defined in Table 2, except that GROWTH is the ratio of current pupils to pupils five years ago, and PUP/POP is pupils per capita.

[b]Feldstein (1978), Eq. 1.1, p. 82. Feldstein also estimates seven other expenditure equations, using different estimation methods or different data sets. Estimated coefficients of PRICE range from -0.940 to -1.599.

[c]Ladd (1975), Eq. 1, p. 151. Ladd also estimates two other expenditure equations with nonlinear refinements on RESIDENT to account for property tax base composition. Estimated coefficients of PRICE are -0.586 and -0.653.

[d]Not reported. Failure to include the coefficient of CAT in Ladd's reported Eq. 1 is probably a typographical error.

large price effect on expenditures. Price elasticities are -0.5 and -1.0 in the reported equations, and range up to -1.6 in one unreported Feldstein equation.*

The primary purpose of this report is to see whether the Feldstein and Ladd results hold up in another state with a GTB plan in effect. A secondary purpose is to extend the Feldstein and Ladd models somewhat by controlling for additional variables that may be related to taste for or cost of education.

*Studies by Stern (1973), Grubb and Michelson (1974), and others also use the Massachusetts data, but the behavioral equations are specified in such a way that no independent price effect is estimated.

II. THE DATA

Table 2 lists the variables used in our analysis. The policy variables--those under the control of the school district, the state, or the federal government--are E, PRICE, STBLK, STCAT, and FEDCAT. We seek to explain school districts' choice of E, operating expenditure per pupil. E is the dependent variable in all of our equations.

PRICE TO SCHOOL DISTRICTS

Under a guaranteed tax base plan, such as that now in use in Michigan, a school district may be able to "buy" an additional dollar of educational expenditure for less than a dollar of locally raised revenue. In Michigan in 1974/75, for example, the state guaranteed $39,000 of assessed value per student up to a school operating tax rate of 25 mills.* Consider a district with assessed value (AV) less than $39,000 per student. In 1974/75 it faced an increasing block price schedule such as that shown in Fig. 1. If its demand for educational expenditure were D_1 it would buy E_1 dollars per pupil. Up to $975 (25 mills x $39 per mill), each additional dollar of E would cost it AV/39 of locally raised revenue, which the state would match with 1 - AV/39 of matching aid. That is, the PRICE of E to the district is AV/39. PRICE is also sometimes expressed in terms of the state matching rate m or the state share s; Table 3 compares the three formulations.**

---

*In 1973/74, the corresponding values are $38,000 and 22 mills. In 1975/76, the state guaranteed $42,400 for the first 20 mills and $38,250 for the next 7 mills.

**Feldstein is somewhat unclear as to how the PRICE variable is calculated in his study. On p. 80 he says, ". . . price is equal to one minus the local marginal matching rate." This is incorrect unless he means "state share" when he says "matching rate." But footnote 10 says, ". . . price is calculated as the ratio of local current 'reimbursible expenditures' (as defined by ch. 70 of the Massachusetts laws) to the sum of that expenditure and the state matching aid implied by the legislative formula." This describes the correct calculation if "local expenditures" means "expenditures from local revenue."

Table 2

LIST OF VARIABLES

Variable	See Note: A	B	C	D	Brief Description	Dimension
E	X				Operating expenditure per pupil	Dollars per pupil
RESIDLAG				+	Residuals from last year's equation	Dollars per pupil
TIME				+	1 in first year, ...5 in fifth year	Years
GROWTH				-	Pupils this year/pupils last year	Dimensionless
STBLK	X				Unrestricted state aid per pupil	Dollars per pupil
STCAT	X			+	Categorical state aid per pupil	Dollars per pupil
FEDCAT	X			+	Federal aid per pupil	Dollars per pupil
CAT	X			+	Sum of STCAT and FEDCAT	Dollars per pupil
PRICE	X	X		-	Cost to district of one-dollar addition to E	Dimensionless
TAX				-	Deductions from state aid to district in fifth year	Dollars per pupil
DUM5				-	1 in fifth year; 0 otherwise	Dimensionless
AV		X		+	Assessed value per pupil	$ thousand per pupil
P2		X	X	-	Product of PUP/HHLD, RESIDENT, and DISTVAL	Pupils per household
PUP/HHLD		X	X	-	Pupils per household	Pupils per household
RESIDENT		X	X	-	Proxy for residential fraction of total assessed value	Dimensionless
DISTVAL		X	X	-	Ratio of median to mean value of owner-occupied housing	Dimensionless
Y			X	+	Median household income	Dollars
OWNOCC			X	-	Ratio of owner-occupied to total dwelling units	Dimensionless
PRIVATE			X	-	K-12 private school pupils per capita	Dimensionless
POVERTY			X	-	Fraction of families in poverty status	Dimensionless
PROF			X	+	Fraction of employed persons 16 or older working as professional, technical, or kindred	Dimensionless
MINOR			X	+	Nonwhite fraction of population	Dimensionless
URBAN			X	+	Urban fraction of population	Dimensionless
OLDER			X	-	Fraction of population 55 or older	Dimensionless
STABIL			X	+	Fraction of population 55 or older still in same house as 5 years ago	Dimensionless
POPULAT			X	+	Population	Persons

NOTE: A = policy variable.
B = component of price to median household.
C = Census variables fixed year to year at 1970 values.
D = expected sign.

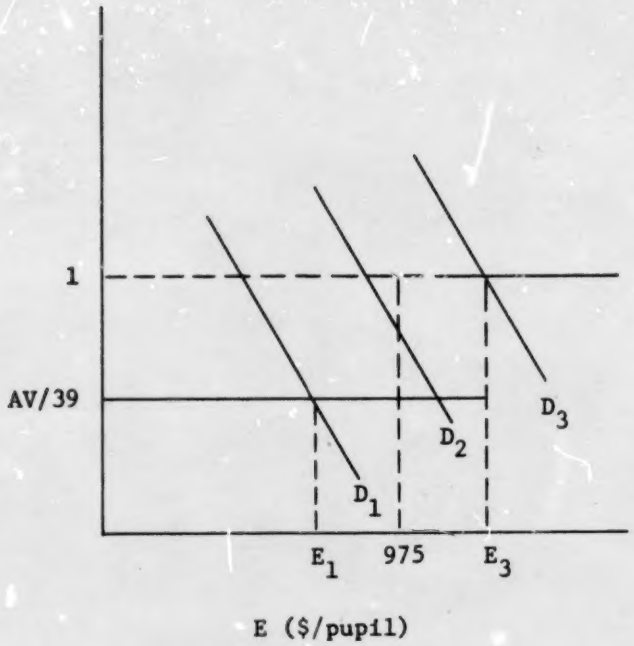

Fig. 1--The price of expenditures per pupil to Michigan districts, 1973/74

Table 3

THREE FORMULATIONS OF PRICE TO A SCHOOL DISTRICT

Local Revenue	State Aid	Total Revenue	PRICE
AV/GTB[a]	1 - AV/GTB	1	AV/GTB
1	m	1 + m	1/(1 + m)
1 - s	s	1	1 - s

[a]GTB is the guaranteed assessed value per pupil, in thousands of dollars.

A district with a higher demand D_3 would buy E_3 dollars per pupil. Additional dollars would have to come entirely from local revenue, so PRICE equals 1. However, the state would match the first 25*AV dollars per pupil of local revenue with 25*(39-AV) dollars per pupil. This amount is a lump-sum transfer to the district that does not affect PRICE of additional E.

What about districts whose demand curves, like D_2 in Fig. 1, pass through the gap in the price schedule? We can recognize them because their tax rate for school operation OM (for "operating millage") will exactly equal the limit for state matching aid. Strictly speaking, their PRICE is indeterminate. One more dollar of E would cost them a full dollar of local revenues; one less would save them only AV/39. The PRICE that we would like to use in estimating the demand curve falls someplace in between, and in principle should be estimated simultaneously with the coefficients of the demand curve. For now we treat D_2 districts like D_1 districts, setting PRICE equal to AV/GTB. In the estimation section, we check to see how the results are affected if instead we treat D_2 districts like D_3 districts, or if we exclude them from the sample.

Districts with AV greater than or equal to GTB receive no matching aid; for them, PRICE equals 1 for all values of E. Also, in 1971/72 and 1972/73, when the foundation plan was in effect, PRICE equals 1 for all districts. Finally, transitional guarantees based on 1972/73 revenues were effective for some districts in the first GTB year, 1973/74; PRICE in 1973/74 equals 1 for these districts.

PRICE TO REPRESENTATIVE HOUSEHOLD (P2 *PRICE)

If school boards are responsive to pressures from residents in their districts, then it is not only PRICE to the district that matters, but price to the households in the district. Price to the representative household, P_1, will be greater or less than PRICE to the district as the household's assessed valuation (in $1000), V_1, is greater or less than AV:

$$P_i = \text{PRICE} * (V_i/AV)$$
$$\equiv \text{PRICE} * P2 \ .$$

This is algebraically equivalent to

$$P_i = \text{PRICE} * (V_i/AV) * (n/n) * (N/N) * (V_r/V_r)$$
$$= \text{PRICE} * (N/n) * (V_r/V) * (V_i/(V_r/n)) \ ,$$

where N is the number of students,

n is the number of households,

V is the total assessed valuation (=AV *N),

V_r is the assessed valuation of residential property.

If we take the representative household to be the one in the median-valued, owner-occupied house, all the terms in this expression are variables defined in Table 2:

$$P_i = \text{PRICE} * \text{PUP/HHLD} * \text{RESIDENT} * \text{DISTVAL} \ .$$

If the individual components of P_i affect demand only through their effect on P_i, all the terms in this expression should enter the demand equation with the same exponent. This may be too restrictive, however, in light of the fact that some of the terms may pick up taste or wealth effects on demand in addition to pure price effects. Consequently, we begin with an unrestrictive specification that allows separate exponents on the different components of P_i, and, in the estimation section, test various restrictions on the exponents.

STATE AND FEDERAL AID (STBLK, STCAT, FEDCAT, TAX, AND DUM5)

STBLK is unrestricted state aid to the district exclusive of marginal matching aid; STCAT is categorical state aid; FEDCAT is federal aid. All three are measured in per-pupil terms.

We can distinguish several forms of state aid:

1. Marginal matching grants under a GTB plan to districts wi AV less than GTB and OM less than the matching limit.

2. Inframarginal matching aid under a GTB plan to districts with AV less than GTB and OM greater than the matching limit.

3. Unrestricted nonmatching aid under a transitional guarantee in 1973/74.

4. Unrestricted nonmatching aid under a foundation plan in 1971/72 and 1972/73.

5. Categorical aid.

The first shows up as a lower PRICE to the district, and is not counted again in STBLK. We add together 2 through 4 to get STBLK. The fifth is STCAT.

Michigan's state aid program was not fully funded in 1975/76. To make up the shortfall, each district was "taxed" four percent of the sum of its local revenues and state noncategorical aid. The "tax" was deducted from the state's noncategorical aid payments to the district. If the "tax" exceeded those payments, the balance was deducted from the state's categorical aid payments to the district.

The shortfall was not recognized until after districts had established their 1975/76 local property tax rates. Thus 1975/76 expenditures were planned in response to the aid program's parameters. However, the subsequent reductions in their revenues probably affected the observed values of E. Accordingly, we used the aid program's parameters to compute the 1975/76 values for PRICE, STBLK, and STCAT (gross of the subsequent deductions) for each district and introduced additional variables to control for the effects of the tax on E.

We tried a variable, TAX, which equaled the "tax" levied on each district. It performed well in the time-series, cross-sectional analyses, but took on the wrong sign (+) in cross-sectional analyses of the fifth-year data. These results are not surprising. Because local revenues and state noncategorical aid account for nearly 90 percent of districts' total revenues, TAX is nearly proportional to E. An accurate estimate of the cross-sectional net effect of TAX on E

would be obtained only if our other variables very precisely controlled for the effects of all the other factors influencing E, and our data are not that good.

Alternatively, we used a dummy variable, DUM5, for the fifth year in the time-series, cross-sectional analyses. It performed about as well as did TAX.

In any event, the coefficients of PRICE and the aid variables were about the same whether TAX or DUM5 was included.

OTHER VARIABLES

The other variables used in our analysis are self-explanatory. Many of them (from PUP/HHLD down in Table 2) are calculated using 1970 census figures, and thus are fixed from year to year at 1970 values. This does not introduce much error for those that change only slowly over time--probably all except median household income, Y, and population, POPULAT. Even Y and POPULAT will cause no problems to the extent that they are uniformly trended over time for all districts. In the cross-section regressions, a uniform trend will affect only the scale of the Y and POPULAT coefficients. In the time-series, cross-section regressions, the effect of the trend will be picked up by our TIME variable. Non-uniform changes in these variables across districts will bias our estimated coefficients because of errors in variables problems.

Summary statistics for all variables are shown in Table 4.

Table 4

SUMMARY STATISTICS

	1971/72		1972/73		1973/74		1974/75		1975/76	
Variable	Mean	Std Dev	Mean	Std Dev	Mean	Std Dev	Mean	Std Dev	Mean	Std Dev
E	791.21	120.35	863.94	133.28	957.53	147.97	1089.80	167.76	1197.40	186.79
GROWTH	--	--	1.0073	.0381	1.0005	.0416	1.0000	.0370	1.0013	.0353
STBLK	343.58	115.95	364.01	133.43	252.92	225.47	134.47	221.96	102.83	207.60
STCAT	44.83	25.89	55.14	26.65	71.60	25.46	75.50	29.50	64.28	40.25
FEDCAT	31.04	30.58	34.88	32.87	36.07	35.18	45.14	44.17	57.52	47.56
CAT	75.87	45.80	90.02	49.87	107.66	51.48	120.64	61.99	121.80	74.00
PRICE	1.0000	0.0	1.0000	0.0	.8241	.2705	.6976	.2758	.7039	.2529
AV	16.863	7.921	18.507	8.966	20.351	10.587	23.042	12.839	25.792	14.850
TAX	0.0	0.0	0.0	0.0	0.0	0.0	0.0	0.0	8.16	17.38
DUM5	0.0	0.0	0.0	0.0	0.0	0.0	0.0	0.0	1.00	0.0
P2	.6097	.5585								
PUP/HHLD	.9342	.2884								
RESIDENT	.7101	.6864								
DISTVAL	.9003	.0588								
Y	8923	2468								
OWNOCC	.8126	.0833								
PRIVATE	.0230	.0241								
POVERTY	.0849	.0481								
PROF	.1158	.0492								
MINOR	.0284	0761								
URBAN	.3541	.3951								
OLDER	.1779	.0558								
STABIL	.5745	.0809								
POPULAT	17667	72088								

NOTE: Variables from PUP/HHLD down are fixed at their 1970 census values.

III. ESTIMATED EXPENDITURE EQUATIONS

All equations are log-linear with all variables except TIME, TIME*2, and DUM5 in natural logs.

CROSS-SECTIONAL REGRESSIONS

Table 5 presents a simple reestimation of Feldstein's equation for each of our five years. For the most part, the results are roughly consistent across years and agree fairly well with Feldstein's estimates. The main exception is estimated price elasticity, which is not significantly different from zero in 1973/74, and significant but quite small (-0.15 and -0.19) in 1974/75 and 1975/76. In addition, the coefficient for STBLK is significantly negative, contrary to expectations, in the first two years.

Table 6 shows results for our expanded specification, which controls for the effect of additional variables that may be related to taste for or cost of education. The first regression in each year is an unrestrictive specification in which each component of price to the representative household, P_i = PRICE *PUP/HHLD *RESIDENT *DISTVAL, has its own coefficient. In addition, we include a separate AV term so that its coefficient is not constrained to be equal to the negative of the coefficient of RESIDENT.

We tested several restrictions on the coefficients of the components of P_i. To test the hypothesis that they all are equal, we ran regressions using only the composite variable P_i. To test equality of the coefficients of PUP/HHLD, RESIDENT, and DISTVAL, we ran regressions using PRICE and P2. These were compared with regressions in which all four components enter separately. In addition, for each of these three cases, we both included and excluded a separate AV term. The results of these tests for 1974/75 are summarized in Table 7; results for 1973/74 and 1975/76 are similar. The inclusion of a separate AV term is strongly compelled by the data, and so are separate coefficients for PRICE and P2. On the other hand, the hypothesis that the coefficients of PUP/HHLD, RESIDENT, and DISTVAL are equal is not

Table 5

CROSS-SECTIONAL REGRESSIONS: FELDSTEIN SPECIFICATION

Variable	1971/72	1972/73	1973/74	1974/75	1975/76
CONSTANT	4.7465	4.8374	4.3525	3.9846	4.2781
	(16.18)	(18.14)	(17.05)	(16.77)	(16.88)
GROWTH		-1.0491	-.8433	-.6875	-.7183
		(8.51)	(7.41)	(5.87)	(5.44)
STBLK	-.0254	-.0213	.0040	.0364	.0359
	(3.54)	(3.60)	(.86)	(9.30)	(9.27)
CAT	.0933	.1343	.1368	.1267	.1006
	(9.04)	(11.64)	(10.82)	(11.38)	(9.43)
PRICE			.0414	-.1525	-.1902
			(1.31)	(5.00)	(5.36)
AV	.1238	.1367	.1358	.2287	.2368
	(6.23)	(7.36)	(6.16)	(8.81)	(8.29)
PUP/HHLD	-.0874	-.0130	-.0652	-.0887	-.1015
	(3.11)	(.50)	(2.50)	(3.84)	(4.09)
RESIDENT	.0518	.0720	.0373	-.0016	-.0030
	(3.08)	(4.74)	(2.46)	(.12)	(.20)
Y	.1491	.1217	.1642	.1722	.1591
	(4.79)	(4.25)	(5.88)	(6.81)	(5.91)
PRIVATE	-.1066	-.2214	-.1766	.0650	.0603
	(.47)	(1.04)	(.82)	(.33)	(.28)
R-Squared	.484	.576	.574	.637	.575
SEE	.100	.093	.094	.087	.095

NOTE: All variables are in natural logs; t-statistics are given in parentheses.

Table 6

CROSS-SECTIONAL REGRESSIONS: EXPANDED SPECIFICATION

Variable	1971/1972		1972/1973			1973/1974			1974/1975			1975/1976		
CONSTANT	5.3859 (18.14)	5.5485 (19.98)	5.5748 (19.88)	5.6516 (21.69)	5.7266 (44.19)	5.2280 (18.59)	5.3760 (20.38)	5.4974 (37.20)	5.0450 (18.74)	5.1414 (20.41)	5.5645 (37.03)	5.4389 (18.72)	5.5451 (20.34)	5.8050 (33.56)
RESIDLAG					.8361 (36.37)			.8846 (30.79)			.8123 (28.24)			.8513 (25.44)
GROWTH			-.8620 (6.73)	-.9649 (6.78)	-.2987 (4.57)	-.6956 (6.00)	-.6918 (5.95)	-.5115 (7.82)	-.5631 (4.81)	-.5661 (4.84)	-.6217 (8.95)	-.5765 (4.39)	-.5792 (4.42)	-.4186 (5.03)
STBLK	-.0210 (3.10)	-.0211 (3.11)	-.0170 (3.04)	-.0168 (3.03)	-.0157 (5.67)	.0039 (.92)	.0048 (1.11)	-.0057 (2.33)	.0288 (7.53)	.0291 (7.65)	.0103 (4.36)	.0271 (7.12)	.0271 (7.12)	.0160 (8.25)
STCAT	.0321 (3.86)	.0313 (3.77)	.0692 (5.87)	.0688 (5.86)	.0506 (8.64)	.0878 (6.24)	.0894 (6.36)	.0767 (9.72)	.0691 (6.38)	.0687 (6.45)	.0481 (7.45)	.0397 (4.77)	.0401 (4.83)	.0311 (5.90)
FEDCAT	.0391 (5.79)	.0396 (5.86)	.0395 (6.30)	.0395 (6.32)	.0341 (10.93)	.0220 (3.59)	.0223 (3.63)	.0300 (8.67)	.0211 (3.63)	.0209 (3.60)	.0211 (6.11)	.0351 (4.83)	.0353 (4.86)	.0308 (6.68)
PRICE						.0240 (.80)	.0179 (.60)	.0440 (2.62)	-.1202 (4.11)	-.1233 (4.23)	-.0345 (1.96)	-.1570 (4.64)	-.1578 (4.68)	-.0909 (4.22)
SEV	.0704 (3.22)	.0724 (3.41)	.0830 (4.07)	.0819 (4.14)	.0665 (6.75)	.1014 (4.39)	.1084 (4.81)	.0824 (6.51)	.1924 (7.40)	.1960 (7.63)	.1288 (8.35)	.2034 (7.27)	.2069 (7.43)	.1650 (9.31)
P2		-.0257 (1.41)		.0031 (.19)	-.0133 (1.59)		-.0085 (.51)	-.0056 (.59)		-.0157 (1.04)	-.0097 (1.09)		-.0103 (.63)	-.0091 (.89)
PUP/HHLD	-.0646 (2.33)		-.0097 (.37)			-.0518 (1.94)			-.0416 (1.69)			-.0420 (1.58)		
RESIDENT	-.0235 (1.23)		.0061 (.34)			-.0076 (.44)			-.0145 (.92)			-.0101 (.60)		
DISTVAL	.0152 (.21)		-.0142 (.21)			.0521 (.73)			.0137 (.21)			.0484 (.67)		
Y	.1125 (3.24)	.0905 (2.79)	.0733 (2.21)	.0647 (2.10)	.0632 (4.13)	.0978 (2.93)	.0743 (2.38)	.0747 (4.28)	.0948 (3.00)	.0802 (2.74)	.0708 (4.07)	.0652 (1.92)	.0484 (1.53)	.0438 (2.19)
OMBOCC	-.0825 (1.42)	-.0974 (1.69)	-.0744 (1.34)	-.0780 (1.42)	-.1037 (3.80)	-.1002 (1.78)	-.1186 (2.12)	-.1246 (3.98)	-.1880 (3.56)	-.1978 (3.79)	-.1990 (6.42)	-.1649 (2.87)	-.1781 (3.13)	-.1910 (5.30)
PRIVATE	-.2648 (1.19)	-.1771 (.81)	-.1255 (.59)	-.0974 (.47)	-.1793 (1.73)	-.1973 (.91)	-.1048 (.49)	-.1386 (1.16)	-.1025 (.50)	-.0449 (.22)	-.1076 (.90)	-.1050 (.47)	-.0331 (.15)	-.0687 (.50)
POVERTY	-.0279 (1.48)	-.0369 (2.03)	-.0390 (2.19)	-.0425 (2.48)	-.0480 (5.64)	-.0228 (1.25)	-.0322 (1.83)	-.0427 (4.34)	-.0287 (1.68)	-.0345 (2.10)	-.0374 (3.83)	-.0456 (2.46)	-.0526 (2.95)	-.0493 (4.37)
PROF	.0534 (3.20)	.0552 (3.35)	.0472 (2.97)	.0489 (3.13)	.0506 (6.51)	.0248 (1.52)	.0260 (1.61)	.0317 (3.50)	.0172 (1.11)	.0187 (1.20)	.0265 (2.95)	.0246 (1.48)	.0251 (1.53)	.0265 (2.56)
MINOR	.3606 (4.01)	.3760 (4.19)	.3129 (3.64)	.3160 (3.69)	.3675 (6.63)	.3085 (3.57)	.3233 (3.74)	.3644 (7.52)	.3519 (4.29)	.3619 (4.44)	.4453 (9.18)	.3689 (4.19)	.3839 (4.39)	.4175 (7.53)
URBAN	.1459 (5.41)	.1454 (5.40)	.1128 (4.35)	.1137 (4.41)	.1359 (10.58)	.1440 (5.31)	.1455 (5.36)	.1503 (9.89)	.0954 (3.75)	.0959 (3.78)	.1020 (6.77)	.0980 (3.42)	.0984 (3.44)	.1011 (5.58)
OLDER	-.0094 (.40)	.0016 (.07)	-.0049 (.22)	.0011 (.05)	.0001 (.01)	-.0278 (1.21)	-.0173 (.80)	-.0097 (.81)	-.0264 (1.20)	-.0192 (.94)	-.0266 (2.20)	-.0135 (.58)	-.0063 (.29)	-.0116 (.84)
STABIL	.0188 (.47)	.0243 (.61)	-.0573 (1.43)	-.0564 (1.41)	-.0017 (.08)	-.0467 (1.17)	-.0398 (.99)	-.0256 (1.14)	.0419 (1.13)	.0457 (1.24)	.0504 (2.31)	.0356 (.87)	.0406 (1.00)	.0568 (2.12)
POPULAT	-.0103 (1.36)	-.0070 (.96)	-.0123 (1.73)	-.0111 (1.61)	-.0047 (1.37)	-.0171 (2.30)	-.0143 (1.96)	-.0111 (2.73)	-.0137 (2.05)	-.0117 (1.79)	-.0079 (2.04)	-.0146 (1.95)	-.0122 (1.68)	-.0107 (2.32)
R-Squared	.606	.6032	.657	.6569	.9154	.650	.6461	.8892	.687	.6858	.8896	.639	.636	.855
SEE	.089	.089	.085	.085	.042	.087	.087	.049	.082	.082	.048	.088	.088	.056

NOTE: All variables are in natural logs; t-statistics are given in parentheses.

Table 7

F-STATISTICS FOR TESTS OF RESTRICTIONS ON THE COEFFICIENTS OF COMPONENTS OF PRICE TO REPRESENTATIVE HOUSEHOLD

	Against This One:				
Test of This Specification . . .	1	2	3	4	5
1. P_1					
2. P_1, AV	128				
3. PRICE, P2	73				
4. PRICE, P2, AV	71	10	58		
5. PRICE, PUP/HHLD, RESIDENT, DISTVAL	26		2.4[a]		
6. PRICE, PUP/HHLD, RESIDENT, DISTVAL, AV	36	4.0	20	.9[a]	55

[a]All values are significant at the .01 level except these two, which are not significant at the .05 level.

rejected at the 0.05 level. Consequently, we adopt the "streamlined" specification (PRICE P2, and AV) and report these equations in the second column for each year in Table 6.

We also performed an informal test of the effect of our treatment of districts whose demand curves pass through the gap in the price schedule (D_2 districts in Fig. 1). Our standard treatment considers them to be the same as D_1 districts, with PRICE = AV/GTB and STBLK = 0. We test this against two alternatives:

- Consider them to be the same as D_3 districts, with PRICE = 1 and STBLK = OM *(GTB - AV).
- Omit them from the sample.

It makes surprisingly little difference which treatment is used. (The 1974/75 results for the two alternatives are compared with those for the standard treatment in Table 10, below.)

The third equation for each year in Table 6 is an attempt to account for the relationship between years in cross-sectional regressions. Here we include the residual from the previous year's regression, RESIDLAG, in this year's equation. The rationale is that there are many influences on a school district's expenditure decisions that we do not capture, even with our extended list of variables, and that these influences tend to be stable from year to year. The residual from last year's equation is an estimate of these idiosyncratic effects.

Somewhat more formally, if our model is

$$\begin{aligned} y_{it} &= X_{it}\beta + u_{it}\,, \\ u_{it} &= \rho\, u_{i,t-1} + \epsilon_{it}\,, \\ \epsilon_{it} &\sim N(0, \sigma^2)\,, \end{aligned} \tag{1}$$

then we can rewrite

$$y_{it} = X_{it}\beta + \rho\, u_{i,t-1} + \epsilon_{it}\,.$$

Our procedure uses last year's residuals as estimates of $u_{i,t-1}$ and runs the augmented regression.

We note particularly the following features of Table 6. The additional variables introduced in our expanded specification perform as expected (with the exception of OLDER and STABIL, for which the results are mixed and generally insignificant). The anomalous results noted in Table 5 hold for our expanded specification as well: The coefficient of STBLK is negative in 1971/72 and 1972/73; the coefficient of PRICE is positive in 1973/74, and negative but very small in 1974/75 and 1975/76.

POOLED TIME-SERIES, CROSS-SECTION REGRESSIONS

Table 8 gives the results of regressions using pooled data for all five years. Ordinary least squares (O'S) regression is appropriate

Table 8

POOLED TIME-SERIES, CROSS-SECTION REGRESSIONS

	Method					
Variable	OLS		VARCOMP		AUTOCORR	
CONSTANT	5.274 (45.91)	5.271 (45.88)	5.732 (22.26)	5.729 (22.24)	5.802 (25.91)	5.800 (25.95)
TIME	.0066 (0.59)	.0081 (0.71)	.0420 (7.74)	.0421 (7.73)	.0412 (8.38)	.0410 (8.33)
TIME**2	.0162 (7.28)	.0158 (7.10)	.0103 (10.01)	.0103 (9.94)	.0109 (11.99)	.0109 (11.96)
GROWTH	-.6404 (10.68)	-.6412 (10.69)	-.3393 (10.29)	-.3397 (10.30)	-.3187 (11.26)	-.3190 (11.26)
STBLK	.0137 (7.92)	.0137 (7.93)	.0059 (6.64)	.0059 (6.66)	.0051 (5.08)	.0051 (5.08)
STCAT	.0536 (12.33)	.0538 (12.36)	.0162 (5.85)	.0163 (5.90)	.0173 (6.50)	.0174 (6.55)
FEDCAT	.0307 (10.73)	.0307 (10.71)	.0213 (9.56)	.0212 (9.55)	.0180 (7.99)	.0179 (7.97)
PRICE	-.0447 (3.63)	-.0449 (3.64)	-.0302 (5.07)	-.0303 (5.08)	-.0203 (2.90)	-.0205 (2.92)
TAX		-.0079 (4.35)		-.0076 (9.58)		-.0079 (13.14)
DUM5	-.0573 (4.56)		-.0531 (9.69)		-.0554 (13.24)	
AV	.1367 (15.08)	.1365 (15.05)	.1057 (11.42)	.1050 (11.55)	.0953 (10.20)	.0950 (10.17)
P2	-.0061 (1.59)	-.0063 (1.63)	.0047 (1.73)	.0044 (1.62)	.0032 (1.28)	.0029 (1.15)
Y	.0690 (4.98)	.0691 (4.99)	.0431 (1.35)	.0434 (1.36)	.0422 (1.52)	.0424 (1.53)
OWNOCC	-.1608 (6.57)	-.1604 (6.55)	-.1917 (3.45)	-.1909 (3.44)	-.1952 (4.06)	-.1945 (4.05)
PRIVATE	-.1349 (1.41)	-.1354 (1.41)	-.1022 (0.45)	-.1018 (0.45)	-.0755 (0.38)	-.0772 (0.39)
POVERTY	-.0399 (5.11)	-.0401 (5.14)	-.0389 (2.19)	-.0392 (2.20)	-.0376 (2.42)	-.0379 (2.45)
PROF	.0332 (4.59)	.0332 (4.60)	.0329 (1.91)	.0331 (1.92)	.0358 (2.39)	.0360 (2.41)
MINOR	.3825 (9.90)	.3829 (9.90)	.4914 (5.43)	.4919 (5.43)	.4960 (6.30)	.4967 (6.32)
URBAN	.1309 (10.96)	.1312 (10.98)	.1199 (4.50)	.1203 (4.51)	.1282 (5.52)	.1287 (5.55)
OLDER	-.0151 (1.57)	-.0151 (1.56)	.0058 (0.26)	.0060 (0.27)	.0101 (0.51)	.0101 (0.51)
STABIL	.0186 (1.05)	.0185 (1.04)	.0239 (0.58)	.0236 (0.57)	.0346 (0.96)	.0346 (0.96)
POPULAT	-.0144 (4.58)	-.0144 (4.57)	-.0048 (0.66)	-.0048 (0.65)	-.0054 (0.85)	-.0054 (0.84)
Untransformed R-Squared	.822	.822	.804	.804	.803	.802
SEE	.088	.088	.092	.092	.092	.092
Transformed R-Squared			.939	.939	.957	.957
SEE			.051	.051	.043	.043
$\hat{\rho}$			.8603	.8603	.8841	.8835

NOTE: All variables except TIME, TIME**2, and DUM5 are in natural logs; t-statistics are given in parentheses.

only if errors are uncorrelated. But we know from the regressions including RESIDLAG in Table 6 that errors are in fact highly correlated from year to year, so OLS estimates are inefficient and estimated t-statistics are apt to be badly biased. Nevertheless, the pooled OLS regression does serve two important purposes in our analysis.

First, we used the OLS regression to test the form of the time variable. The reported equation includes both linear and squared terms in time (TIME and TIME**2). The inclusion of TIME**2 is strongly compelled by testing against an equation with TIME only; F = 212.1. On the other hand, the data offer no reason to include a TIME**3 term; for this test, F = 1.6.

Second, we used the residuals from the OLS equation to estimate starting values for the covariance matrices used in our other two estimation methods. These two are generalized least squares (GLS) procedures that differ in the form of the covariance matrix that is assumed.

The commonly specified variance components model (VARCOMP) hypothesizes that the error term includes a district-specific component that is constant from year to year:

$$u_{it} = \mu + \varepsilon_{it} ,$$

where i indexes districts and t indexes years. We assume $E(\mu_j,\mu_j) = E(\mu_i,\varepsilon_{it}) = E(\varepsilon_{it},\varepsilon_{is}) = E(\varepsilon_{it},\varepsilon_{jt}) = 0$ for any $i \neq j$ and $t \neq s$. Then, if we array our observations first by district, and within districts by time, the covariance matrix for this model is

$$E(u\ u') = \sigma^2\ V = \sigma^2 \begin{bmatrix} A & 0 & \cdots & 0 \\ 0 & A & \cdots & 0 \\ \vdots & \vdots & & \vdots \\ 0 & 0 & \cdots & A \end{bmatrix}$$

where

$$A = \begin{bmatrix} 1 & \rho & \rho & \rho & \rho \\ \rho & 1 & \rho & \rho & \rho \\ \rho & \rho & 1 & \rho & \rho \\ \rho & \rho & \rho & 1 & \rho \\ \rho & \rho & \rho & \rho & 1 \end{bmatrix}$$

A consistent estimate of ρ is given by

$$\hat{\rho} = \frac{5 \Sigma_i \bar{u}_i^2}{5 \Sigma_i \bar{u}_i^2 + \Sigma_i \Sigma_t (\hat{u}_{it} - \bar{u}_i)^2} \qquad (2)$$

where $\hat{u}_{it}$ are residuals from the pooled OLS regression and $\bar{u}_i = (1/5)\Sigma_t \hat{u}_{it}$. In our case we calculate $\hat{\rho} = .7998$ and use that estimate in V to calculate the estimated coefficients:

$$b = (X' V^{-1} X)^{-1} X' V^{-1} y .$$

We iterated, using the residuals from each pass to reestimate $\hat{\rho}$ via Eq. (2) for the next pass, until $\hat{\rho}$ changed by less than .0001. The estimated coefficients for the final pass are shown in the middle column of Table 8.

The VARCOMP method assumes that the district-specific component of the error term does not change at all over time. This may be unduly rigid. A softer specification is that the errors are correlated over time (AUTOCORR) as specified in Eq. (1) above. GLS is again an appropriate estimation method. Now,

$$A = \begin{bmatrix} 1 & \rho & \rho^2 & \rho^3 & \rho^4 \\ \rho & 1 & \rho & \rho^2 & \rho^3 \\ \rho^2 & \rho & 1 & \rho & \rho^2 \\ \rho^3 & \rho^2 & \rho & 1 & \rho \\ \rho^4 & \rho^3 & \rho^2 & \rho & 1 \end{bmatrix}$$

and our initial estimate of ρ is

$$\hat{\rho} = \frac{\Sigma_i \Sigma_{t=2}^{5} \hat{u}_{i,t} \hat{u}_{i,t-1}}{\Sigma_i \Sigma_{t=2}^{5} \hat{u}_{i,t} \hat{u}_{i,t}} = .8325 .$$

We iterated, updating our estimate of ρ in each pass until $\hat{\rho}$ converged. The results for the final pass are shown in the last column of Table 8.

The R-squared and standard error of estimate (SEE) values reported in Table 8 require some explanation. Those labeled "untransformed" correspond to the R-squared value conventionally reported for OLS regressions. They are based on the sum of squared errors from using the reported coefficients to compute fitted values of the dependent variable. The untransformed SEE is the square root of $\hat{\sigma}_u^2$, the estimated variance of the *composite* error term in our model.

The "transformed" values are based on the sum of squared errors from the regression using GLS transformed variables. The transformed SEE is the square root of $\hat{\sigma}_\varepsilon^2$, the estimated variance of ε alone.

The untransformed values are the appropriate measure of how well we are fitting any conceivable sample of Michigan school districts.* The transformed values are a better measure of how well we are fitting our particular sample, since they take account of the fact that we know something about the stability of errors over time for the districts in our sample.

STRUCTURAL CHANGE

The introduction of the GTB plan might have changed the structure of school districts' decision processes. At the extreme, there might have been structural changes in districts' responses to all the variables. A less extreme possibility is a change in the structure of

*Since we have included all districts for which we have usable data, "any conceivable sample" is a somewhat artificial construct. To generate such a sample, one would have to run the process that generated our data, using a new set of values for the independent variables.

districts' responses to the PRICE variable, which existed only implicitly prior to 1973/74. And even if the long-term structure of the PRICE response did not change, there might have been a short-run structural change in the PRICE responses during the first year or two of the new plan.

To test for structural change in districts' PRICE responses, we estimated time-series, cross-sectional models which had four price variables: PRICE and PRICE times dummy variables for years 3, 4, and 5. We compared the results with those obtained for similar models that had a common PRICE term across years. In each case, we entered all the independent variables listed in Table 8 except DUM5 or TAX. Every regression was estimated by three methods: OLS, VARCOMP, and AUTOCORR. We used our initial estimates of $\hat{\rho}$, .7998 and .8325, in the VARCOMP and AUTOCORR runs, respectively. Table 9 gives F-statistics for the addition variance explained by allowing the PRICE coefficient to vary across years.

The hypothesis that no change in the PRICE response occurred over the five years cannot be rejected at the .05 level. In no case

Table 9

F-STATISTICS FOR TESTS FOR STRUCTURAL CHANGE IN THE PRICE RESPONSE

Specification	OLS	VARCOMP	AUTOCORR
All variables shown in Table 8 except DUM5	1.43	2.47	1.67
All variables shown in Table 8 except TAX	1.17	2.40	1.88

Note: $\hat{\rho}$ = .7998 in each VARCOMP run, and .8325 in each AUTOCORR run.

does allowing the PRICE coefficient to vary between pre-reform and post-reform years and among post-reform years add significant explanatory power to the equation. We can assume, then, that the structure

of districts' responses to PRICE has not changed. Lacking annual data on the "taste" variables, we cannot test for structural changes in their effects on districts' behavior. But it does not seem reasonable to believe that the influence of variables not directly related to the GTB plan would have changed with its introduction, while the structure of districts' responses to PRICE--the variable directly affected by the introduction of the GTB plan--remained constant.

DISCUSSION

We concentrate our discussion on the results for the AUTOCORR method in Table 8. In contrast to the cross-sectional results, these make simultaneous use of all the information at our disposal. Of the three pooled equations, they are based on the most plausible error structure. In any event, the AUTOCORR results are not greatly different from those for other methods.

We find again that the coefficient of PRICE is very small, and only marginally significant at best.* As a practical matter, it matters little whether it is significant or not, since a PRICE elasticity of -.02 is hardly useful as a policy tool. For example, changing the matching rate from 0 to 1, a 50 percent reduction in PRICE, would increase spending only one percent.

The coefficient of STBLK is also very small. The implication is that a one percent increase in unrestricted state aid--about $1 per pupil for the average district--would cause a .005 percent increase in spending--about 6 cents per pupil.

The coefficients for the other forms of aid are much larger. They imply that expenditures are increased by about 32 percent of the amount of state categorical aid, and about 38 percent of the amount of federal aid.

We can think of two econometric problems that might be affecting our results, although they do not necessarily explain why the coefficients of PRICE and STBLK are so small. First, it is clear that in

*The work of Park and Micthell (1978) on estimating the autocorrelated error model with trended data suggests that the reported t-statistics may be overstated by a factor of perhaps 1.5.

GTB years, 1973/74 through 1975/76, the levels of PRICE and STABLK depend to some extent on the realized level of district demand for expenditure. This is not a problem if our independent variables explain demand perfectly, but it is if demand is subject to random fluctuation. A positive random error will result in higher expenditure. It will also move some districts across the step in the PRICE function (Fig. 1), increasing both PRICE and STBLK for those districts. The resultant positive correlation between the error term and PRICE and STBLK will bias the estimated coefficients of these variables in a positive direction. This would help to explain why our PRICE coefficient is so close to zero, but it does nothing to explain why the coefficient of STBLK is so small; in fact, it suggests that the true coefficient should be even smaller than the one we estimate.

Two pieces of evidence suggest that the bias is not very large. First, the correlation between the error terms and PRICE and STBLK should be highest for districts near the step in the PRICE schedule. When we eliminated the districts at the step (Table 10), it made very little difference. Second, we tried a two-stage estimation procedure treating PRICE and STBLK as endogenous; it, too, failed to improve the results or to change them very much.

The second econometric problem is multicollinearity among our independent variables. In each year STBLK and PRICE are given by the state aid formula as piecewise linear functions of AV. The resultant correlations among the log forms of the variables in the pooled data are as follows:

	PRICE	STBLK	AV
PRICE	1.		
STBLK	.767	1.	
AV	.204	-.333	1.

The multiple correlation among all three is given by the square root of the coefficient of determination for a regression of one on the other two. For the regression of STBLK on PRICE and AV, this equals

Table 10

ALTERNATIVE TREATMENTS OF DISTRICTS WHOSE DEMAND CURVES PASS THROUGH THE PRICE SCHEDULE GAP, 1974/75

	Treatment[a]		
Variable	(1)	(2)	(3)
CONSTANT	5.1414	5.1592	5.2122
	(20.41)	(19.52)	(20.42)
GROWTH	-.5661	-.5530	-.5875
	(4.84)	(4.56)	(4.96)
STBLK	.0291	.0302	.0253
	(7.65)	(7.58)	(6.77)
STCAT	.0697	.0698	.0701
	(6.45)	(6.19)	(6.40)
FEDCAT	.0209	.0195	.0211
	(3.60)	(3.19)	(3.59)
PRICE	-.1233	-.1248	-.1046
	(4.23)	(4.13)	(3.65)
AV	.1960	.2008	.1789
	(7.63)	(7.61)	(7.20)
P2	-.0157	-.0132	-.0168
	(1.04)	(.83)	(1.09)
Y	.0802	.0760	.0814
	(2.74)	(2.48)	(2.75)
OWNOCC	-.1978	-.2087	-.1884
	(3.79)	(3.86)	(3.58)
PRIVATE	-.0449	-.0894	-.0978
	(.22)	(.41)	(.48)
POVERTY	-.0345	-.0357	-.0309
	(2.10)	(2.07)	(1.85)
PROF	.0182	.0103	.0231
	(1.20)	(.64)	(1.51)
MINOR	.3619	.3587	.3819
	(4.44)	(4.27)	(4.64)
URBAN	.0959	.1094	.1153
	(3.78)	(4.08)	(4.53)
OLDER	-.0192	-.0166	-.0212
	(.94)	(.77)	(1.03)
STABIL	.0457	.0339	.0468
	(1.24)	(.88)	(1.25)
POPULAT	-.0117	-.0141	-.0127
	(1.79)	(2.05)	(1.93)
R-Squared	.6858	.7035	.6778
SEE	.082	.083	.083
N	451	410	451

NOTES: (1) D_2 districts are treated like D_1 districts.
(2) D_2 districts are omitted.
(3) D_2 districts are treated like D_3 districts.

[a]See Fig. 1.

.916. This collinearity will not bias our coefficients, but it will reduce the precision with which we estimate them. And a multiple R of .916, though troublesome, does not seem high enough to account for our results.

Feldstein's and Ladd's estimates for Massachusetts suggest that state matching and block grants substantially affect educational expenditures. They report price elasticities of -.5 to -1.0 and block grant elasticities of .03 to .07. (See Table 1.) The Michigan data, however, provide no support for their conclusions. The differences between our results and theirs may partially reflect the econometric techniques we employed. The 1974/75 and 1975/76 cross-sectional regressions reported in Table 5 are the closest we can come to replicating Feldstein's specification.* There we estimate an expenditure elasticity with respect to state block grants of .036, very close to Ladd's estimate and reasonably close to Feldstein's. But even there, we obtain price elasticities of -.15 and -.19, less than one-fifth of Feldstein's comparable result and roughly one-third of Ladd's.

It may be that Michigan school district decisionmakers or voters, or both, simply do not generally value educational expenditures.** Even if school quality enters their preferences, they may base allocative decisions on some measure of quality that is poorly correlated (at least in their minds) with educational expenditures. An immense volume of research has so far failed to establish a relationship between school spending and student achievement. (See, for example, Averch et al. (1974).) Michigan's school districts may not have tried, or may not have succeeded, when they did try, to convince the voters to the contrary. The "price" inherent in the GTB plan would then be irrelevant to their decisions.

---

*Michigan did not have matching grants in 1971/72 or 1972/73, and the results obtained for state block grants in those years pertain to the effects of such grants in the context of a foundation plan. The 1973/74 results may be contaminated by the transitional guarantees and/or by delays in districts' responses to the introduction of a new state school finance plan.

**The Michigan constitution provides for a 15 mill school levy. Any higher rate must be approved by the voters. Trivial numbers of districts levied the minimum rate in the years covered by our data.

Alternatively, school districts or voters may equate the "cost" of education with the local property tax *rate*. As Feldstein notes, the education community in general and the school finance community in particular have confused the school tax rate with the "price" of education. The GTB plan provides every district (below the matching ceiling) with identical marginal trade-offs between local tax rates and expenditures. Those may not be the relevant trade-offs, but they may be what actually motivate budgetary decisions in most districts. There would then be no "price" response because there would be no perceived "price" differences among districts.

Another possibility is that the lack of mechanisms for shifting local tax revenues among public agencies may dampen price response. The GTB plan may make education a "better buy" in one district than in another relative to private and other public goods and services. In principle, other public agencies coextensive with the former district (e.g., cities) should reduce their property tax rates while the district increases its rate, the net result being a reallocation of funds from private and other public uses to the schools. But what if the other public agencies do not reduce their local property tax rates? Given "too much" spending in the noneducational public sector, the public will opt for "too little" educational spending, at least in the short run.

Finally, it should be noted that our data pertain to the first three years of Michigan's GTB plan. The Massachusetts data examined by Feldstein and by Ladd were for the fifth year of that state's plan. It is possible, of course, that Michigan districts will exhibit greater price responsiveness in the future.

In sum, the Michigan data provide no evidence that state matching or block grants do much to stimulate school district expenditures. For whatever reason, school districts in Michigan have not (as yet) responded appreciably to changes in the implicit price of school expenditures.

REFERENCES

Averch, Harvey A., Stephen J. Carroll, Theodore S. Donaldson, Herbert J. Kiesling, and John Pincus, *How Effective is Schooling? A Critical Review of Research*, Educational Technology Publications, Englewood Cliffs, N.J., 1974.

Feldstein, Martin, "Wealth Neutrality and Local Choice in Public Education," *American Economic Review*, Vol. 65, March 1975, pp. 75-89.

Grubb, W. Norton, and Stephan Michelson, *States and Schools*, Lexington Books, D. C. Heath and Co., Lexington, Mass., 1974.

Ladd, Helen F., "Local Education Expenditures, Fiscal Capacity, and the Composition of the Property Tax Base," *National Tax Journal*, Vol. 28, June 1975, pp. 145-158.

Park, R. E., and B. M. Mitchell, *Estimating the Autocorrelated Error Model with Trended Data*, The Rand Corporation, R-2273-NIE/RC, March 1978.

Stern, David, "Effects of Alternative State Aid Formulas on the Distribution of Public School Expenditures in Massachusetts," *Review of Economics and Statistics*, Vol. 55, February 1973, pp. 91-97.